work has hitherto given us so effective a series of coloured petrographic illustrations. We are thus not quite sure about the description of the pyroxene-andesite from Bohemia on plate v., because the drawing so closely resembles the rock of Tichlowitz, with its brown hornblende in the groundmass, its monoclinic pyroxene, and its patches of zeolites as the only pale constituents. Again and again we could name the locality of the rock selected from the accurate details of the illustration; and when we turn to the descriptive text, we find very little room for criticism. G. A. J. C.

Inflammation. An Introduction to the Study of Pathology. Being the reprint (revised and enlarged) of an article in Prof. Allbutt's "System of Medicine." By Prof. J. George Adami. Pp. xvi+240. (London: Macmillan and Co., Ltd., 1907.) Price 5*s*. net.

REPRINTS in book form of articles appearing in larger volumes are not always desirable, but in the present instance so much has been added to the matter as virtually to constitute a new work. We congratulate Prof. Adami heartily on the successful issue of an arduous task; no one knows how difficult until he attempts to write on inflammation. The subject of inflammation, forming, as it does, the fundamental basis of pathology, and it might be said also of the science and practice of medicine, is beset with difficulties. The literature on it is voluminous and bewildering, and pathologists owe a debt of gratitude to Prof. Adami for having the courage to attack it. The matter is divided into sections; the first gives a general survey of the inflammatory process, the second deals with the various factors of the process—the part played by the leucocytes, the exudate, the blood-vessels, the nervous system, cells of the part, and the temperature changes; the third section deals with general considerations, and includes a chapter on the principles of treatment of the inflammatory state. Every statement made is based on published work, to which the reference is appended (and the book therefore forms a valuable bibliography on the subject of inflammation), and critical additions and summaries are liberally interspersed. The book is well and sufficiently illustrated, and no student of pathology can do without it. R. T. HEWLETT.

Notes on Maritime Meteorology. By Commander M. W. Campbell Hepworth, C.B. Pp. viii+90; 7 plates. (London: George Philip and Son, Ltd., 1907.) Price 2*s*. 6*d*. net.

THIS work consists of papers contributed to societies and institutions between 1883 and 1900, compiled while the author was on active service afloat. Two of them, occupying nearly half the book, are of a more general nature than the rest, and deal with meteorology as a factor in naval warfare and with the value of meteorological observations at sea. The author contends that, given two opposing fleets equal in all respects, "the victory in a series of engagements shall be to the fleet in the direction of whose movements meteorology shall have given the greatest aid," and some striking instances are cited of the value of weather knowledge. The other papers are of a more special character, and relate chiefly to the navigation of the Indian and Pacific oceans. Taken in connection with the useful charts dealing with the marine meteorology of those oceans published by the Admiralty and the Meteorological Office, the results of investigations by so experienced a seaman and so keen an observer as the author of the work in question will be of great interest and value to those now afloat.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Mulattos.

MR. H. G. WELLS, in his interesting book "The Future in America" (1906), tells (pp. 269–270) a story at second-hand which apparently, however, he accepts as accurate in perfect good faith. I transcribe the facts as they were given to him:—

"A few years ago a young fellow came to Boston from New Orleans. Looked all right. Dark—but he explained that by an Italian grandmother. Touch of French in him too. Popular. Well, he made advances to a Boston girl—good family. Gave a fairly straight account of himself. Married."

The offspring of the marriage was a son:—

"Black as your hat. Absolutely negroid. Projecting jaw, thick lips, frizzy hair, flat nose—everything."

In this case Mr. Wells observes:—"The taint in the blood surges up so powerfully as to blacken the child at birth beyond even the habit of the pure-blooded negro."

This is, at any rate, ultra-Mendelian. Such a story would hardly be told and repeated unless it corresponded to popular belief. What one would like to have is precise evidence that such cases actually occur. If verifiable, it would be of great importance both on scientific and political grounds. I find, however, nothing resembling it in such authorities as I am able to consult. No such case is mentioned by either Darwin or Delage, though neither would have been likely to pass over such a striking instance of reversion had it been known to him. Sir William Lawrence, in his "Lectures on Physiology, Zoology, and the Natural History of Man" (1822), a book still worth consulting, has industriously collected (pp. 472–484) all the facts available at the time about mulattos, but has no instance of the kind.

The problem involved is thus stated by Galton ("Natural Inheritance," p. 13):—"A solitary peculiarity that blended freely with the characteristics of the parent stock, would disappear in hereditary transmission." He then discusses the case of a European mating in a black population:—"If the whiteness refused to blend with the blackness, some of the offspring of the white man would be wholly white and the rest wholly black. The same event would occur in the grandchildren, mostly, but not exclusively, in the children of the white offspring, and so on in subsequent generations. Therefore, unless the white stock became wholly extinct, some undiluted specimens of it would make their appearance during an indefinite time, giving it repeated chances of holding its own in the struggle for existence." *Mutatis mutandis*, the same law would hold for a black mating in a white population.

Lawrence quotes a single case (p. 279) in which a refusal to blend certainly existed:—"A negress had twins by an Englishman: one was perfectly black, with short, woolly curled hair; the other was light, with long hair." He also points out that in "mixed breeds" "children may be seen like their grandsires, and unlike the father and mother," a fact observed by Lucretius.

"Fit quoque, ut interdum similes existere avorum
Possint, et referant proavorum sæpe figuras."

On the other hand, according to Lawrence, there was a legal process in the Spanish colonies of South America by which a mulatto could claim a declaration that he was, at any rate politically, free from any taint of black blood. Of Quinterons, who were one-sixteenth black, he says:—"It is not credible that any trace of mixed origin can remain in this case," and even of Tercerons, who were one-quarter black, "in colour and habit of body they cannot be distinguished from their European progenitors." He says (p. 274) that Jamaica Quadroons "are not to be distinguished from whites." But "there is still a contamination of dark blood, although no longer visible. It is said to betray itself sometimes in a relic of the peculiar strong smell of the great-grandmother." If these statements can be relied upon, Galton's hypothetical law does not appear

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to apply to mulattos, and some doubt is thrown on the case cited by Wells. On the other hand, Lawrence quotes from the Philosophical Transactions (" v., 55 ") a case of two negroes who had a white child, the paternal grandfather being white. This seems purely Mendelian.

November 25. W. T. THISELTON-DYER.

Specific Stability and Mutation.

THE desire to be as brief as possible has led, I fear, to some obscurity in the sentences quoted by Sir William Thiselton-Dyer (p. 77) from my letter of October 17. The meaning will perhaps be clearer if I explain the precise significance which I attached to the words " appear " and " occurrence."

By the occurrence of a mutation in one of the higher plants I meant the production of a seed capable of germination and containing an embryo with definitely different potentialities from those of its parent. The appearance of a mutation, on the other hand, implies that such a seed has germinated and given rise to a plant recognisably different from other members of the species. My contention is that the conditions of cultivation are such as to allow of the safe germination and growth of plants which would have no chance of survival under natural conditions. It is therefore possible that mutations may *occur* as frequently under natural conditions as under cultivation. This being so, it does not appear to me to be an abuse of language to state that the assumption that cultivation causes the occurrence of mutations is one which requires proof. In support of this assumption Sir William Thiselton-Dyer brings forward certain evidence. With much of this evidence I was already familiar, but it did not appear to me to amount to satisfactory proof of the current position. The authority of eminent breeders is quoted for the fact that, as soon as one new variety of a cultivated species has been obtained, a host of others immediately follow. But the explanation of this may be that the breeder, as soon as he has obtained a single novelty, immediately crosses it—deliberately or by accident —with the original type, thus giving rise to endless new combinations. R. H. LOCK.

Botany School, Cambridge, December 2.

THAT mutations inevitably appear sooner or later under cultural conditions is not an assumption, but a fact. That they do so only casually under natural conditions, and usually fail to perpetuate themselves, equally seems to me not an assumption, but a fact. If, as Mr. Lock seems to argue, there is an equal chance of their *occurrence* in either case, then their *appearance* should be more frequent in nature than in cultivation, because the former has a larger population to work with. But it is not so. I therefore conclude with Darwin that cultivation introduces some provocative condition which is lacking (or latent) in nature. What that condition is seems to me a very important subject for research.

December 5. W. T. THISELTON-DYER.

The Winding of Rivers.

WITH your permission I would like to make a few remarks on the winding of rivers, which is at present being discussed in your pages. My observations were made while fishing, and my remarks refer to the rivers of our own country, and may not apply to rivers of greater volume. But first I would like to point out an objection to Prof. J. Thomson's experiments. In Prof. Thomson's paper in the report of the British Association for 1876 no details of the conditions of the experiment are given, but Sir Oliver Lodge in his letter (NATURE, November 28) says Prof. Thomson's model had a wooden bed. Now it is very evident that we must be careful in drawing conclusions from experiments made under these conditions. That wooden bed, however carefully made, would not be of the shape that nature would have given it, and any deviation from nature's shape would cause unnatural currents. It, however, does seem probable that something of the nature of Prof. Thomson's diagonal under-tow will exist even in river-shaped beds.

The whole question of the flow of water in river beds is extremely complicated. This is evidenced by the contrary results of the observations of your correspondents. But little consideration is sufficient to show that this must be so; the variables are so many. We have, for instance, variations in the curvature of the bend, in the velocity of the water, and in the formation of the bed of the river, which we must remember is dug out and shaped by flood water for flood water, and is but little altered as the river falls in volume. Take, for instance, the case supposed to be represented by Prof. Thomson's model. Here, with a certain curvature and a certain velocity of flow, we can easily imagine the formation of the diagonal under-tow. But if we were to increase the velocity of the flow this cross under-current would decrease and ultimately cease, and when a certain relation of velocity to curvature was arrived at we would get the conditions referred to in Mr. R. D. Oldham's letter in NATURE of November 21, where he says:—" Sand and even pebbles may be thrown up to the surface of the water near the outer bank of the stream, and where the waters have overflowed the banks pebbles may be found lying on the dry ground after the flood has passed away."

In most of the rivers I know which flow in gravel beds, where they are constantly cutting away their banks, the main flow is more sinuous at low level than when in flood. At low level the main stream runs into the pools at the bends on the deep or concave bank, and as the deep sides of the successive pools are on opposite banks of the river, the stream has to cross its bed between the successive pools. While in flood the swiftest flowing part, on the surface at least, is near midstream, but the formation of the bed at the place and the flow above and below may alter this in some cases. After the flood has fallen, the river bed it has shaped has an infinite variety of forms at different places, and the flow of the water at any part must be studied with reference to that particular part, and to the part above which has determined the cross-section and velocity of the water coming to it, and also to the formation of the part below which determines its escape.

There is one very common type of flow which frequently presents itself in varying forms in rivers which alter considerably in volume from time to time. After the flood has fallen the river becomes, so to speak, divided into streams and pools. Over the shallows the water runs rapidly, while in the pools it moves slowly and somewhat irregularly. The streams coming into the pools flow next the concave banks, and come into the pools with some velocity, which is soon lost in the slower movements in the pools. The streams thus lose their kinetic energy, which is converted into potential energy, raising the level of the water at the place where the stream loses its velocity. From this part of the pool, in addition to the stream flowing down the pool, a reverse current is started which flows back on the inner side of the pool, flowing to the upper end of it, where it curves round and flows downwards alongside the main stream. Part of the back current is no doubt due to the inflowing main current causing an induced current, but it seems to be mainly due to the loss of kinetic energy of the stream, causing a rise of the level of the water where its velocity is destroyed.

As to the cutting and wearing away of the banks of rivers, that is mainly the result of eddies formed by the flowing water meeting with obstructions, such as stones, tree roots and stems, inequalities in the banks, &c., or even by water impinging on water. One of the deepest pools in a river I observed was entirely dug out of its gravel bed by eddies produced by the main river meeting a large tributary stream at right angles and mingling their waters in turbulent eddies; and it seems probable that the excavation of the deep pools generally found at the foot of waterfalls have been greatly aided by the eddies formed by the falling water meeting the quieter water of the pools.

The common practice in this country of protecting the banks of rivers by means of little piers or " tooks " to throw the water off them, and into the middle of the bed of the stream, generally results in failure, because the piers cause eddies, and deep pools endangering the banks are frequently dug out by these eddies; and while these piers tend to throw the water to the other side of the channel, yet the sloping bed throws it back and causes it to strike

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